superintendent of the "Nautical Almanack" and secretary of the Board of Longitude, and in 1827, on the resignation of Sir Humphry Davy, was spoken of as a probable successor to his office of President of the Royal Society, Davies Gilbert, however, being chosen. He died in 1829, at the age of 56, and his character was thus drawn by his intimate friend Sir Humphry Davy :—"A man of universal erudition and almost universal accomplishments. Had he limited himself to any one department of knowledge, he must have been first in that department. But as a mathematician, a scholar, and a hieroglyphist, he was eminent ; and he knew so much that it was difficult to say what he did not know."

Sir Humphry Davy's brilliant career, and especially that portion of it which contributed so greatly to the fame and success of the Institution with which he was connected, is drawn in detail by his biographer ; and the failings in his character and in his life which obscured its lustre to his contemporaries are in no way concealed. The following contrast of the characters of Davy and of his pupil and successor, Faraday, will be read with interest :—"Whenever a true comparison between these two nobles of the Institution can be made, it will probably be seen that the genius of Davy has been hid by the perfection of Faraday. Incomparably superior as Faraday was in unselfishness, exactness, and perseverance, and in many other respects also, yet certainly in originality and in eloquence he was inferior to Davy, and in love of research he was by no means his superior." As early as 1804, when Davy was only twenty-six, Dr. Dalton consulted him as to the best mode of preparing his lectures, and described him as "a very agreeable and intelligent young man, the principal failing in whose character as a philosopher is that he does not smoke ;" and within two or three years from that time he had made the discoveries which have immortalised his name.

Dr. Bence Jones does not carry down the history of the Royal Institution beyond 1814, when it became as closely associated with Faraday's career as it had previously been with Davy's. We have seen what were the primary objects for the promotion of which the Institution was founded ; and we know also the great work which it effected during the first ten years of its existence. These special purposes soon gave way to the effort, as our author expresses it, after striving to be fashionable ; and the fashionable element has continued to be the most prominent feature in its subsequent life to the present day. Something is, no doubt, gained by making scientific subjects one of the ordinary topics of conversation in West End *salons ;* the continuation of the History of the Royal Institution, which will have to be written twenty years hence, will show whether this object is compatible with the carrying on of original investigations which will add to the sum of our knowledge of the laws of Nature.

OUR BOOK SHELF

Une Expérience relative à la Question de la Vapeur Vésiculaire. Par M. J. Plateau. (Brussels : F. Hayez.)

THE elder Saussure, and after him De Luc, considered it to be an established fact that clouds are formed of little hollow globules, which Saussure designated vesicular vapours, or vesicles. These vesicles, having a structure similar to the soap bubble, were assumed to be capable of floating in the atmosphere and of remaining suspended in it so long as their physical condition was unaltered. When they became resolved into drops of water they formed rain. The same structure was assigned to the cloud formed by the condensation of the vapour of boiling water in air colder than itself. M. Plateau has endeavoured to put this view of the vesicular constitution of vapour to the test of experiment. With this view he has taken advantage of a method devised by M. Duprez, for inverting a wide tube (20mm. in diameter) full of water, so that the water may remain suspended in the tube. By means of a narrow tube drawn out at one end, so as to present an orifice of 0·4mm. in diameter, he succeeded in obtaining small hollow globules of water of less than a millimetre in diameter, and transporting them under the free surface of the water, suspended in the wide tube. As soon as contact was established with that surface, the little bubble became detached, and the air which it contained penetrating into the liquid, mounted through it. The experiment, on being several times repeated, gave always the same result. M. Plateau has applied this method to the cloud formed when water is boiled in free air. "Let us imagine," he says, "that at a certain distance from the surface of the water suspended in the wide tube, a current of visible vapour of water arises. If this vapour is composed of vesicles, each of them which comes into contact with the liquid surface must introduce into the water a microscopic bubble of air, which will immediately begin to ascend, so that the whole will form in the water of the tube a cloud which will rise slowly in it, and alter its transparency." In making the experiment, no cloud was produced, and M. Plateau concludes, in conformity with the view now generally held by physicists, that the vesicular state of vapour has no real existence. He discusses objections which may be raised to his experiment, such as the possible solution of the bubbles of air in the water, the bursting of the bubbles at the surface of the water and the escape of the air contained in them, or their rolling under the surface of the water till they reach the margin of the tube and thus get away ; and shows satisfactorily that these objections do not invalidate the result at which he has arrived.

Chemical Notes for the Lecture Room, on Heat, Laws of Chemical Combination, and Chemistry of non-Metallic Elements. By Thomas Wood, Ph.D., F.C.S. Pp. 181. (London : Longmans, Green, and Co.)

ON reading this volume the author's intention is plainly manifest ; the book has been written principally for the use of students preparing for the matriculation examination at the University of London. It has been written as concisely as possible, rendering the task of "cramming" the subject more easy of attainment. For such a purpose we certainly can recommend this book ; but for beginners who wish to study chemistry we think it has several faults. One of them is that such a comparatively large amount of the book is devoted to the subsidiary subject, Heat, almost a quarter of the text being thus occupied. The article on thermometers, for instance, occupies no less than nine pages, which strikes us as being rather out of proportion to the remainder of the book. A second fault is the almost complete absence of any such details as would enable a student to repeat the experiments mentioned in the text. This we think is a fault which would tend to make the beginner get up his subject parrot-like, a method which is certainly not to be desired. The chemistry of the non-metallic elements only occupies eighty-five pages of this volume ; the definitions and laws of chemical combination occupy another thirty-eight pages. The explanations, in the majority of instances, are clearly expressed, the facts of the case being stated in as few words as possible. A few of the definitions can scarcely be considered good ; one, in particular, is "that

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a compound of any non-metal with a metal is a salt of a metal." This would, of course, include such bodies as antimonetted and arsenetted hydrogen, hydride of copper, and so on. The definitions of acids and bases, too, are weak. It may almost be inferred that such is the case, by the manner in which the author uses the term acid; N_2O_3 is called nitrous acid; I_2O_5 iodic acid, and, in the same line, $HBrO_3$ bromic acid; B_2O_3 boracic acid, and so on. There is one thing which the author tells us which is a curiosity in chemical history. On page 38 it is stated "some few of the elements receive their symbols *from the names given to them* by the ancients—*e.g.* Iron (Fe.) from *Ferrum*, Sodium (Na.) from *Natrium*." We certainly were under the impression that Sodium was discovered in 1807 by Sir Humphry Davy. A number of questions are appended to the book which will be found very useful to those employed in teaching.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Ocean Currents

SURELY Mr. Ferrel must have misapprehended my arguments, or he would not have advanced the case of the tides against me. Undoubtedly the ocean will sink to its old level when the lifting force of the moon is withdrawn, even though the height to which the waters are raised may not exceed an inch. I agree also with what he says in regard to the improbability of ocean currents being caused by the heaping up of the waters by the winds. I believe that this erroneous view of the matter has done more real mischief to the wind theory than all the arguments advanced by the advocates of the gravitation theory put together. The notion that because the winds are applied to the surface of the ocean they can produce only surface drift is an error of a similar character.

I shall shortly refer to an important point bearing on the influence of rotation overlooked both by Dr. Colding and Mr. Ferrel. In my last paper in the *Phil. Mag.*, October 1871, p. 266, there is a trifling mistake to which I shall also refer.

Edinburgh JAMES CROLL

Science Stations

ALLOW me to say a few words in reply to your editorial of Feb. 29. It does seem to me to be a pity to "run the risk of spoiling a good work" by multiplying suggestions and urging counter claims. It is not quite fair that when biologists start a proposal for obtaining a necessary but costly aid to their studies, the devotees of other sciences should exclaim, "Oh, we must have one, too!" If all speak at once in this way no one will be heard, and we shall get no stations of any sort. Probably the writer of the article is not aware of the expense and requirements of a zoological station, otherwise he would not propose to increase the difficulty by thrusting a meteorological and astronomical observatory on the backs of its promoters, and then observe that "the outlay need not be heavy." It is notorious that there are meteorological and astronomical observatories in almost every part of the globe; but there is nothing of the kind for zoology. Under these circumstances it is to me a disappointment that the suggestion for zoological stations meets with what looks like a somewhat selfish criticism, in place of unqualified support, at the hands of physicists.

As to the station in England, I do not gather from Dr. Dohrn's article that he proposes to separate teaching entirely, or even partially, from the stations. He leaves it alone. "Teaching" can come or go just as those who deal in it may please; but that instruction in rudimentary zoology should be a chief object of the station is a proposal of the same nature as would be that to make use of Greenwich Observatory for giving lessons in the outlines of astronomy, and is not entertained by him for a moment. It no doubt would be a very good thing that students from Cambridge and Oxford and London should spend some time in a zoological station; and it would also be good for others of them to work in a lead or copper mine, or pass a few nights in an astronomical observatory; but we cannot urge the wants of these particular students as any reason for the maintenance of these three things. The primary object for which zoological stations will be erected—one for which it is to be hoped that the Universities, as well as scientific societies and private individuals, will be ready to subscribe money—is the prosecution of science.

We claim for biology now a place of far higher importance in the scheme of human knowledge than she has occupied hitherto. She has proved her claim to the respect and most serious attention of men by the discovery of the principles and detailed laws of evolution—a discovery which has more widely influenced human thought than has any other product of modern science, and must continue long so to do. We are no longer content to see biology scoffed at as "inexact," or gently dropped as "natural history," or praised for her relations to medicine. On the contrary, biology is the science whose development belongs to the day. At this moment she is deserving of more attention, more material aid, more assistance in her young growth, than any other human science. Her youthful performances, her hopeful stride onwards, promise more abundant results from pecuniary aid given to her than can be hoped for from her older sisters, who have "settled in life." If biology requires "stations," she ought to be gladly supplied with them.

I must protest against the notion—urged in your article only, I imagine, as a joke—that without "teaching" (whatever that may mean) there would be danger of a zoological station becoming the home of a narrow zoological clique. The connection is not explained, and I do not think any of your readers will see it. Are observatories the homes of narrow astronomical cliques? Are telescopes without professors liable to become the resort of ambitious young persons, anxious chiefly to discover hydrogen flames where nobody had found them before? I do not believe a bit in the narrow clique suggestion. Teaching bodies breed them much more rapidly and naturally than do working bodies. And as to the privat-docent, anxious to discover a notochord, or the amateur astronomer hunting for hydrogen flames, I would most gladly see them multiplied exceeding abundantly. Would that we could obtain the institution of "privat-docents" in English Universities; by simply erecting a zoological station, would that we could infuse some of their kind of ambition—one of the best a man can have—into English students.

Naples, March 4 E. RAY LANKESTER

[⁂ The article to which our correspondent refers was written by a distinguished biologist.—ED.]

The Etymology of "Whin."

THE following is from Jamieson:—"*Quhyn, Quhin-Stane*, s. i. Green-stone; the name given to basalt, trap, &c. . . . Isl. *hwijna*, resonare, *hwin*, resonans, q. 'the resounding stone.'" "Whin, whinstane, *s.* Ragstone or toadstone."

Whin or gorse, the name given to *Ulex europæus*, common furze, is from a different root, being traced to Welsh *ehwynyn* = weed. A. HALL

YOUR correspondent, Mr. W. R. Bell, will find a derivation given for "Whin" in Jamieson's "Scottish Dictionary," where, under the name *Quhyn* or *Quhin*, it is referred to the "Islandic *hwijna*, resonare," "*hwin*, resonans, q. the resounding stone," probably from the resonance emitted on its being struck. It is in all likelihood the same as the word *whine*, and the root is present in both Celtic and Teutonic tongues, *e.g.*:—

Welsh	*Cwyno*, to complain
Irish	*Cuinead*, mourning (?)
Islandic	*hwijna* (as above)
Danish	*hvine*, to whistle
German	*weinen*, to weep

Compare also the Latin *hinnio*, to neigh.

F. DE CHAUMONT

Oakland, Woolston, March 15

WEBSTER, in his Dictionary (9th edition, 1862), says in explaining this word, which is known all over England, that it means *weeds, gorse, furze, waste growth*, from the Welsh *Chwyn*. That it is "a provincial name given to basaltic rock, and applied by miners to any kind of dark coloured and hard unstratified rock which resists the pick."

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